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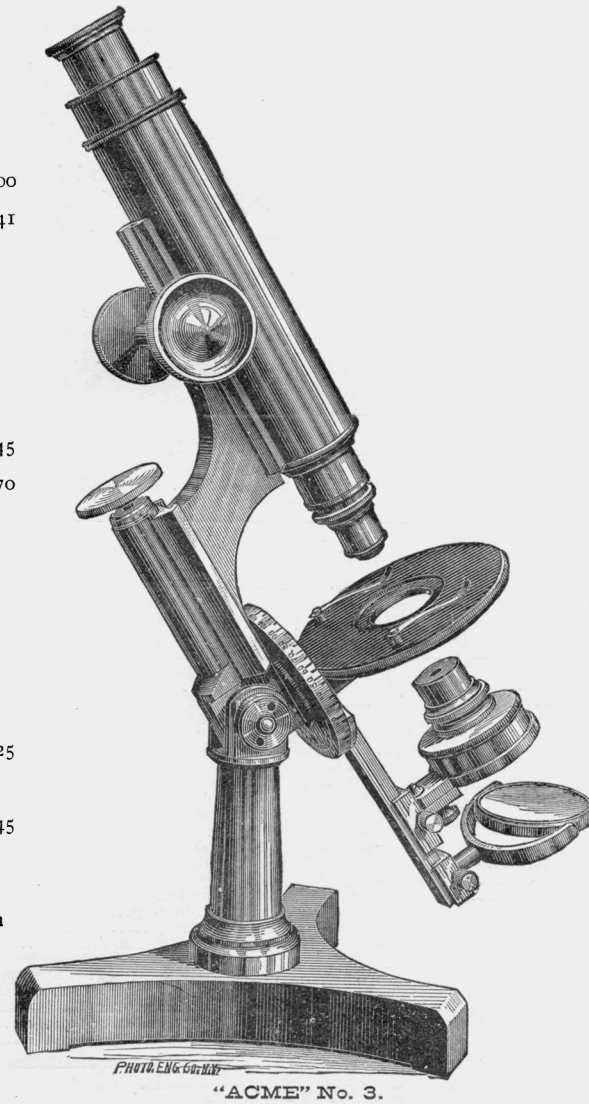
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JOHN MICHELS, Editor.

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The growth of abstract science in this country is perhaps no better illustrated than by the advance which has been made of late years in the various departments of mathematics. It is only a few years since Prof. Peirce was about the only person in the United States who held a position among the original mathematicians of the world, while to-day there are in this country a number of persons whose writings are destined to rank among the classics, and a journal of mathematics of the highest rank is published under the auspices of the Johns Hopkins University and sustained almost entirely by American contributors. Among the best of the abstract writers referred to is Mr. William Ferrel, who has been hitherto best known by his tidal researches, but is now engaged in investigations on the mathematical principles of meteorology. His latest work, just published by the Coast Survey, is now before us, and although nominally consisting only of researches on Cyclones, Waterspouts and Tornadoes, is in reality a valuable contribution to the theory of storms in general.

The Board of Directors of the Ohio Mechanics' Institute have organized a "*Department of Science and Arts*" for the purpose of increasing the usefulness of the Institution. A Section of Mechanics and Engineering under the chairmanship of Professor H. T. Eddy, and one of Chemistry under Professor F. W. Clark, have been arranged. Meetings for the public discussion of scientific subjects will be held once a month, and various other arrangements are in progress which will contribute to the success of the present attempt to provide increased facilities for technical and scientific education for the youths of Cincinnati.

SCIENTIFIC SOCIETIES OF WASHINGTON.

THE BIOLOGICAL SOCIETY, WASHINGTON.—Since our last report the following papers have been read: "Roan Mountain, North Carolina, and its Flora," by Prof. J. W. Chickering, Jr.; "Notes on the Flowering of *Solanum rostratum* and Cassia chamaecrista, with illustrations," by Prof. J. E. Todd; "A Critical Review of Günther's Ichthyology," by Prof. Theodore Gill; "On the Mortality of Marine Animals in the Gulf of Mexico," by Mr. Ernst Ingersoll: "A Statistical View of the Flora of the District of Columbia," by Prof. Lester F. Ward. It is to be regretted that the absence of our Washington correspondent from the meetings deprives us of abstracts of these valuable papers.

THE ANTHROPOLOGICAL SOCIETY.—The Constitution of this society, now in its third year, makes it obligatory upon the President to prepare at the commencement of each year, a summary of the transactions of the organization during the past year. At the close of the first year, the President overlooked this fact, but made ample amends at the commencement of the third year by preparing a pamphlet containing both annual addresses, and copious abstracts of all the papers that had ever been read.

Since our last report, the following papers have been read: "The Savage Mind in the Presence of Civilization," by Prof. Otis T. Mason; "Prehistoric Trephining," by Dr. Robert Fletcher; "Some Superstitions of the Sioux Indians," by Dr. H. Yaddow; "The Chief's Son and the Thunders: An Omaha Myth," by Rev. J. Owen Dorsey.

The design of the first named paper was two-fold: first, to show that the presence of other peoples better furnished and skilled in some respect had always operated as a stimulus in the onward march of civilization; and second to draw attention to the fact that in the treatment of the Indians, Chinese, and Negroes, the phenomena of the past history of civilization were being re-presented. The two latter papers were recitals of exceedingly interesting Indian myths, Dr. Fletcher, who is associated with Dr. Billings in publishing "Index Medicus," having collected all that could be gathered on the subject of prehistoric-trephining, from two years reading, gave an elaborate summary of his investigations.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.—THE SPECTROPHONE.—At the 198th meeting of the Philosophical Society of Washington, Prof. Alexander Graham Bell communicated the announcement of his discovery of the Spectrophone, the latest outgrowth of the Photophone.

In a paper read before the American Association for the Advancement of Science, in which he announced the discovery of the photophone, Mr. Bell ventured the prediction that probably all matter would be found to posses sonorous properties of the same nature as those manifested by the discs used in that instrument. More recent investigations in Europe with gases and liquids have fully verified this prediction. Any liquid or gas placed in a test tube and exposed to the action of a beam of light condensed upon it by a lens can be made, by means of an interrupter, to emit musical tones. This has been shown by Prof. Tyndall in his memoir, to the Royal Society, on Radiant Heat. Some substances thus emit feeble sounds, others stronger ones. Iodine vapor, Nitrogen Oxide and Bromine give very loud sounds. It is found that those substances which emit loud sounds are those which absorb heat in a high degree, and among these lamp-black is especially remarkable. It has been questioned whether such sounds are provoked by the luminous rays or by the dark ones. M. Mercadier expressed the belief that the inciting rays are the red and dark ones. This led Mr. Bell, with the assistance of Mr. Sumner Taintor, to experiment with the sonorous properties of Carbon Disulphide, actuated by the light of the Spectrum.

When lamp-black is exposed to the action of the light of the spectrum it is found to give a sonorous response to all of its rays as far as the middle of the violet, and perhaps beyond. The intensity of the sound, however, varies remarkably in different parts of the spectrum. Taking the rays successively from different parts, from the violet towards the red, the sounds begin very feebly and increase in intensity, reaching a maximum in the ultra red. Beyond that point they suddenly cease. The increase of intensity is very gradual, the decrease very sudden.

Other substances have been experimented with, and while exhibiting similar properties, each has a range of its own. Porous and fibrous substances give loud sounds. Thus, common wool or worsted are found to be very sonorous, but the sounds are obtained wholly from the visible parts of the spectrum and have the maximum intensity in the green. In all substances tried success has resulted, but nearly all have a very short range.

In experimenting with more homogeneous substances of simpler constitution, still more definite results are obtained. The rays of the spectrum are passed through sulphuric ether. Outside of the ultra red is a very narrow band which cause sounds while the other parts fail to produce them. Hydrogen peroxide gives sounds at several places wholly within the visible parts of the spectrum, and these places are found to coincide with the positions of the known absorption bands of that substance. The same is found to be true of Nitrogen Oxide and a solution of Ammonia, Sulphate of Copper, and many other substances. The general law deduced is that *sounds are produced in any substance by the rays which it absorbs.*

Thus a kind of spectrum analysis can be obtained through the intermediation of sound. The principal value of the spectrophone, Mr. Bell believes, will be found in the investigation of absorption bands in the ultra red part of the spectrum.

Mr. William B. Taylor inquired whether the sounds observed from the two absorption bands of ammonia, sulphate of copper, were octaves. Mr. Bell replied that this question had not as yet been investigated.

Mr. G. Brown Goode read portions of a paper on the sword-fish and its allies, which paper will be published in full in the next annual report of the U. S. Fish Commission.

ON THE MODERN DEVELOPMENT OF FARADAY'S CONCEPTION OF ELECTRICITY.*

By Professor Helmholtz.

The majority of Faraday's own researches were connected, directly or indirectly, with questions regarding the nature of electricity, and his most important and most renowned discoveries lay in this field. The facts which he has found are universally known. Nevertheless, the fundamental conceptions by which Faraday has been led to these much-admired discoveries have not been received with much consideration. His principal aim was to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really a progress in general scientific method, destined to purify science from the last remnants of metaphysics. Now that the mathematical interpretations of Faraday's conceptions regarding the nature of electric and magnetic force has been given by Clerk Maxwell, we see how great a degree of exactness and precision was really hidden behind his words, which to his contemporaries appeared so vague or obscure; and it is astonishing in the highest to see what a large number of general theories the methodical deduction of which requires the highest powers of mathematical analysis, he has found by a kind of intuition, with the security of instinct, without the help of a single mathematical formula.

The electrical researches of Faraday, although embracing a great number of apparently minute and disconnected questions, all of which he has treated with the same careful attention and conscientiousness, are really always aiming at two fundamental problems of natural philosophy, the one more regarding the nature of physical forces, or of forces working at a distance; the other, in the same way, regarding chemical forces, or those which act from molecule to molecule, and the relation between these and the first.

The great fundamental problem which Faraday called up anew for discussion was the existence of forces working directly at a distance without any intervening medium. During the last and the beginning of the present century the model after the likeness of which nearly all physical theories had been formed was the force of gravitation acting between the sun, the planets, and their satellites. It is known how, with much caution and even reluctance, Sir Isaac Newton himself proposed his grand hypothesis, which was destined to become the first great and imposing example, illustrating the power of true scientific method.

But then came Oerstedt's discovery of the motions of magnets under the influence of electric currents. The force acting in these phenemena had a new and very singular character. It seemed as if it would drive a single isolated pole of a magnet in a circle around the wire conducting the current, on and on without end, never coming to rest. Faraday saw that a motion of this kind could not be produeed by any force of attraction or repulsion, working from point to point. If the current is able to increase the velocity of the magnet, the magnet must react on the current. So he made the experiment, and discovered induced currents; he traced them out through all the various conditions under which they ought to appear. He concluded that somewhere in a part of the space traversed by magnetic force there exists a peculiar state of tension, and that every change of this tension produces electromotive force. This unknown hypothetical state he called provisionally the electrotonic state, and he was occupied for years and years in finding out what was this electrotonic state. He discovered at first, in 1838, the dielectric polarisation of electric insulators, subject to electric forces. Such bodies show, under the influence of electric forces, phenomena perfectly analogous to those exhibited by soft iron under the influence of the magnetic force. Eleven years later, in 1849, he was able to demonstrate that all ponderable matter is magnetized under the influence of sufficiently intense magnetic force, and at the same time he discovered the phenomena of diamagnetism, which indicated that even space, devoid of all ponderable matter, is magnetizable; and now with quite a wonderful sagacity and intellectual precision Faraday performed in his brain the work of a great mathematician without using a single mathematical formula. He saw with his mind's eye that by these systems of tensions and pressures produced by the dielectric and magnetic polarisation of space which surrounds electrified bodies, magnets or wires conducting electric currents, all the phenomena of electro-static, magnetic, electro-magnetic attraction, repulsion, and induction could be explained, without referring at all to forces acting directly at a distance. This was the part of his path where so few could follow him; perhaps a Clerk Maxwell, a second man of the same power and independence of intellect, was necessary to reconstruct in the normal methods of science the great building, the plan of which Faraday had conceived in his mind and attempted to make visible to his contemporaries.

Nevertheless the adherents of direct action at a distance have not yet ceased to search for solutions of the

* The Faraday Lecture, delivered before the Fellows of the Chemical Society in the Theatre of the Royal Institution, on Tuesday, April 5th, 1881, by Professor Helmholtz. Abstract revised by the author.

electro-magnetic problem. The present development of science, however, shows, as I think, a state of things very favorable to the hope that Faraday's fundamental conceptions may in the immediate future receive general assent. His theory, indeed, is the only existing one which is at the same time in perfect harmony with the facts observed, and which at least does not lead into any contradiction against the general axioms of dynamics.

It is not at all necessary to accept any definite opinion about the ultimate nature of the agent which we call electricity.

Faraday himself avoided as much as he could giving any affirmative assertion regarding this problem, although he did not conceal his disinclination to believe in the existence of two opposite electric fluids.

For our own discussion of the electro-chemical phenomena, to which we shall turn now, I beg permission to use the language of the old dualistic theory, because we shall have to speak principally on relations of quantity.

I now turn to the second fundamental problem aimed at by Faraday, the connection between electric and chemical force. Already, before Faraday went to work, an elaborate electro-chemical theory had been established by the renowned Swedish chemist, Berzelius, which formed the connecting-link of the great work of his life, the systematisation of the chemical knowledge of his time. His starting point was the series into which Volta had arranged the metals according to the electric tension which they exhibit after contact with each other, a fundamental point which Faraday's experiment contradicted with the supposition that the quantity of electricity collected in each atom was dependent on their mutual electro-chemical differences, which he considered as the cause of their apparently greater chemical affinity. But although the fundamental conceptions of Berzelius's theory have been forsaken, chemists have not ceased to speak of positive and negative constituents of a compound body. Nobody can overlook that such a contrast of qualities, as was expressed in Berzelius's theory, really exists, well developed at the extremities, less evident in the middle terms of the series, playing an important part in all chemical actions, although often subordinated to other influences.

When Faraday began to study the phenomena of decomposition by the galvanic current, which of course were considered by Berzelius as one of the firmest supports of his theory, he put a very simple question; the first question, indeed, which every chemist speculating about electrolysis ought to have answered. He asked, What is the quantity of electrolytic decomposition if the same quantity of electricity is sent through several electrolytic cells? By this investigation he discovered that most important law, generally known under his name, but called by him the law of definite electrolytic action.

Faraday concluded from his experiments that a definite quantity of electricity cannot pass a voltametric cell containing acidulated water between electrodes of platinum without setting free at the negative electrode a corresponding definite amount of hydrogen, and at the positive electrode the equivalent quantity of oxygen, one atom of oxygen for every pair of atoms of hydrogen. If instead of hydrogen any other element capable of substituting hydrogen is separated from the electrolyte, this is done also in a quantity exactly equivalent to the quantity of hydrogen which would have been evolved by the same electric current.

Since that time our experimental methods and our knowledge of the laws of electrical phenomena have made enormous progress, and a great many obstacles have now been removed which entangled every one of Faraday's steps, and obliged him to fight with the confused ideas and ill-applied theoretical conceptions of some of his contemporaries. We need not hesitate to say that the more experimental methods were refined, the more the exactness and generality of Faraday's law was confirmed.

In the beginning Berzelius and the adherents of Volta's original theory of galvanism, based on the effects of metallic contact, raised many objections against Faraday's law. By the combination of Nobili's astatic pairs of magnetic needles with Schweigger's multiplicator, a coil of copper wire with numerous circumvolutions, galvanometers became so delicate that the electro-chemical equivalent of the smaller currents they indicated was imperceptible for all chemical methods. With the newest galvanometers you can very well observe currents which would want to last a century before decomposing one milligram of water, the smallest quantity which is usually weighed on chemical balances. You see that if such a current lasts only some seconds or some minutes, there is not the slightest hope to discover its products of decomposition by chemical analysis. And even if it should last a long time the feeble quantities of hydrogen collected at the negative electrode can vanish, because they combine with the traces of atmospheric oxygen absorbed by the liquid. Under such conditions a feeble current may continue as long as you like without producing any visible trace of electrolysis, even not of galvanic polarisation, the appearance of which can be used as an indication of previous electrolysis. Galvanic polarisation, as you know, is an altered state of the metallic plates which have been used as electrodes during the decomposition of an electrolyte. Polarised electrodes, when connected by a galvanometer, give a current which they did not give before being polarised. By this current the plates are discharged again and returned to their original state of equality.

This depolarising current is indeed a most delicate means of discovering previous decomposition. I have really ascertained that under favorable conditions one can observe the polarisation produced during some seconds by a current which decomposes one milligram of water in a century.

Products of decomposition cannot appear at the electrodes without motions of the constituent molecules of the electrolyte throughout the whole length of the liquid. This subject has been studied very carefully, and for a great number of liquids, by Prof. Hittorff, of Münster, and Prof. G. Wiedemann, of Leipsic.

Prof. F. Kohlrausch, of Würzburg, has brought to light the very important fact that in diluted solutions of salts, including hydrates of acids and hydrates of caustic alkalies, every atom under the influence of currents of the same density moves on with its own peculiar velocity, independently of other atoms moving at the same time in the same or in opposite directions. The total amount of chemical motion in every section of the fluid is represented by the sum of the equivalents of the cation gone forwards and of the anion gone backwards, in the same way as in the dualistic theory of electricity, and the total amount of electricity flowing through a section of the conductor corresponds to the sum of positive electricity going forwards and negative electricity going backwards.

This established, Faraday's law tells us that through each section of an electrolytic conductor we have always equivalent electrical and chemical motion. The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion, and accompanies it during all its motions through the interior of the electrolytic fluid. This we may call the electric charge of the atom.

Now the most startling result, perhaps, of Faraday's law is this: If we accept the hypothesis that the elementary substances are composed of atoms we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about on the electrolytic liquid each atom remains united

with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive power, and then the atoms give off their electric charges and become electrically neutral.

Now arises the question, Are all these relations between electricity and chemical combination limited to that class of bodies which we know as electrolytes? In order to produce a current of sufficient strength to collect enough of the products of decomposition without producing too much heat in the electrolyte, the substance which we try to decompose ought not to have too much resistance against the current. But this resistance may be very great, and the motion of the ions may be very slow, so slow indeed that we should need to allow it to go on for hundreds of years before we should be able to collect even traces of the products of decomposition; nevertheless all the essential attributes of the process of electrolysis could subsist. If you connect an electrified conductor with one of the electrodes of a cell filled with oil of turpentine, the other with the earth, you will find that the electricity of the conductor is discharged unmistakably more rapidly through the oil of turpentine than if you take it away and fill the cell only with air.

Also in this case we may observe polarisation of the electrodes as a symptom of previous electrolysis. Another sign of electrolytic conduction is that liquids brought between two different metals produce an electromotive force. This is never done by metals of equal temperature, or other conductors which, like metals, let electricity pass without being decomposed.

The same effect is also observed even with a great many rigid bodies, although we have very few solid bodies which allow us to observe this electrolytic conduction with the galvanometer, and even these only to temperatures near to their melting-point. It is nearly impossible to shelter the quadrants of a delicate electrometer against being charged by the insulating bodies by which they are supported.

In all the cases which I have quoted one might suspect that traces of humidity absorbed by the substance or adhering to their surface were the electrolytes. I show you therefore this little Daniell's cell, in which the porous septum has been substituted by a thin stratum of glass. Externally all is symmetrical at both poles; there is nothing in contact with the air but a closed surface of glass, through which two wires of platinum penetrate. The whole charges the electrometer exactly like a Daniell's cell of very great resistance, and this it would not do if the septum of glass did not behave like an electrolyte. All these facts show that electrolytic conduction is not at all limited to solutions of acids or salts.

Hitherto we have studied the motions of ponderable matter as well as of electricity, going on in an electrolyte. Let us study now the forces which are able to produce these motions. It has always appeared somewhat startling to everybody who knows the mighty power of chemical forces, the enormous quantity of heat and of mechanical work which they are able to produce, and who compares with it the exceedingly small electric attraction which the poles of a battery of two Daniell's cells show. Nevertheless this little apparatus is able to decompose water.

The quantity of electricity which can be conveyed by a very small quantity of hydrogen, when measured by its electrostatic forces, is exceedingly great. Faraday saw this, and has endeavored in various ways to give at least an approximate determination. The most powerful batteries of Leyden jars, discharged through a voltameter, give scarcely any visible traces of gases. At present we can give definite numbers. The result is that the electricity of 1 m.grm. of water, separated and communicated to two balls, 1 kilometre distant, would produce an attraction between them, equal to the weight of 25,000 kilos.

The total force exerted by the attraction of an electrified body upon another charged with opposite electricity is always proportional to the quantity of electricity contained in the attracting as on the attracted body, and therefore even the feeble electric tension of two Daniell's elements, acting through an electrolytic cell upon the enormous quantities of electricity with which the constituent ions of water are charged, is mighty enough to separate these elements and to keep them separated.

We now turn to investigate what motions of the ponderable molecules require the action of these forces. Let us begin with the case where the conducting liquid is surrounded everywhere by insulated bodies. Then no electricity can enter, none can go out through its surface, but positive electricity can be driven to one side, negative to the other, by the attracting and repelling forces of external electrified bodies. This process going on as well in every metallic conductor is called "electrostatic induction." Liquid conductors behave quite like metals under these conditions. Prof. Wüllner has proved that even our best insulators, exposed to electric forces for a long time, are charged at last quite in the same way as metals would be charged in an instant. There can be no doubt that even electromotive forces going down to less than 1-100 Daniell produce perfect electrical equilibrium in the interior of an electrolytic liquid.

Another somewhat modified instance of the same effects is afforded by a voltametric cell containing two electrodes of platinum, which are connected with a Daniell's cell, the electromotive force of which is insufficient to decompose the electrolyte. Under this condition the ions carried to the electrodes cannot give off their electric charges. The whole apparatus behaves, as was first accentuated by Sir W. Thomson, like a condenser of enormous capacity.

Observing the polarizing and depolarizing currents in a cell containing two electrodes of platinum, hermetically sealed and freed of all air, we can observe these phenomena with the most feeble electromotive forces of 1-1000 Daniell, and I found that down to this limit the capacity of the platinum surfaces proved to be constant. By taking greater surfaces of platinum I suppose it will be possible to reach a limit much lower than that. If any chemical force existed besides that of the electrical charges which could bind all the pairs of opposite ions together, and require any amount of work to be vanquished, an inferior limit to the electromotive forces ought to exist, which forces are able to attract the atoms to the electrodes and to charge these as condensers. No phenomenon indicating such a limit has as yet been discovered, and we must conclude, therefore, that no other force resists the motions of the ions through the interior of the liquid than the mutual attractions of their electric charges.

On the contrary, as soon as an ion is to be separated from its electrical charge we find that the electrical forces of the battery meet with a powerful resistance, the overpowering of which requires a good deal of work to be done. Usually the ions, losing their electric charges, are separated at the same time from the liquid; some of them are evolved as gases, others are deposited as rigid strata on the surface of the electrodes, like galvanoplastic copper. But the union of two constituents having powerful affinity to form a chemical compound, as you know very well, produces always a great amount of heat, and heat is equivalent to work. On the contrary, decomposition of the compound substances requires work, because it restores the energy of the chemical forces, which has been spent by the act of combination.

Metals uniting with oxygen or halogens produce heat in the same way, some of them, like potassium, sodium, zinc, even more heat than an equivalent quantity of hydrogen; less oxidisible metals, like copper, silver, platinum, less. We find, therefore, that heat is generated when zinc drives copper out of its combination with the

compound halogen of sulphuric acid, as is the case in a Daniell's cell.

If a galvanic current passes through any conductor, a metallic wire, or an electrolytic fluid, it evolves heat. Mr. Prescott Joule was the first who proved experimentally that if no other work is done by the current the total amount of heat evolved in a galvanic circuit during a certain time is exactly equal to that which ought to have been generated by the chemical actions which have been performed during that time. But this heat is not evolved at the surface of the electrodes, where these chemical actions take place, but it is evolved in all the parts of the circuit, proportionally to the galvanic resistance of every part. From this it is evident that the heat evolved is an immediate effect, not of the chemical action, but of the galvanic current, and that the chemical work of the battery has been spent in producing only the electric action.

If we apply Faraday's law, a definite amount of electricity passing through the circuit corresponds to a definite amount of chemical decomposition going on in every electrolytic cell of the same circuit. According to the theory of electricity the work done by such a definite quantity of electricity which passes, producing a current, is proportionate to the electromotive force acting between both ends of the conductor. You see, therefore, that the electromotive force of a galvanic circuit must be, and is, indeed, proportionate to the heat generated by the sum of all the chemical actions going on in all the electrolytic cells during the passage of the same quantity of electricity. In cells of the galvanic battery chemical forces are brought into action able to produce work; in cells in which decomposition is occurring work must be done against opposing chemical forces; the rest of the work done appears as heat evolved by the current, as far as it is not used up to produce motions of magnets or other equivalents of work.

Hitherto we have supposed that the ion with its electric charge is separated from the fluid. But the ponderable atoms can give off their electricity to the electrode, and remain in the liquid, being now electrically neutral. This makes almost no difference in the value of the electromotive force. For instance, if chlorine is separated at the anode, it will remain at first absorbed by the liquid; if the solution becomes saturated, or if we make a vacuum over the liquid, the gas will rise in bubbles. The electromotive power remains unaltered. The same may be observed with all the other gases. You see in this case that the change of electrically negative chlorine into neutral chlorine is the process which requires so great an amount of work, even if the ponderable matter of the atoms remains where it was.

The more the surface of the positive electrode is covered with negative atoms of the anion, and the negative with the positive ones of the cation, the more the attracting force of the electrodes exerted upon the ions of the liquid is diminished by this second stratum of opposite electricity covering them. On the contrary, the force with which the positive electricity of an atom of hydrogen is attracted towards the negatively charged metal increases in proportion as more negative electricity collects before it on the metal, and the more negative electricity collects behind it in the fluid.

Such is the mechanism by which electric force is concentrated and increased in its intensity to such a degree that it becomes able to overpower the mightiest chemical affinities we know of. If this can be done by a polarized surface, acting like a condenser, charged by a very moderate electromotive force, can the attractions between the enormous electric charges of anions and cations play an unimportant and indifferent part in chemical affinity?

You see, therefore, if we use the language of the dualistic theory and treat positive and negative electricities as two substances, the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom with different force. Potassium, sodium, zinc, must have strong attraction to a positive charge; oxygen, chlorine, bromine to a negative charge.

Faraday very often recurs to this to express his conviction that the forces termed chemical affinity and electricity are one and the same. I have endeavored to give you a survey of the facts in their mutual connection, avoiding, as far as possible, introducing other hypotheses, except the atomic theory of modern chemistry. I think the facts leave no doubt that the very mightiest among the chemical forces are of electric origin. The atoms cling to their electric charges and the opposite electric charges cling to the atoms. But I don't suppose that other molecular forces are excluded, working directly from atom to atom. Several of our leading chemists have begun lately to distinguish two classes of compounds, molecular aggregates and typical compounds. The latter are united by atomic affinities, the former not. Electrolytes belong to the latter class.

If we conclude from the facts that every unit of affinity of every atom is charged always with one equivalent either of positive or of negative electricity, they can form compounds, being electrically neutral, only if every unit charged positively unites under the influence of a mighty electric attraction with another unit charged negatively. You see that this ought to produce compounds in which every unit of affinity of every atom is connected with one and only with one other unit of another atom. This is, as you will see immediately, indeed, the modern chemical theory of quantivalence, comprising all the saturated compounds. The fact that even elementary substances, with few exceptions, have molecules composed of two atoms, makes it probable that even in these cases electric neutralization is produced by the combination of two atoms, each charged with its electric equivalent, not by neutralization of every single unit of affinity.

But I abstain from entering into mere specialties, as for instance, the question of unsaturated compounds; perhaps I have gone already too far. I would not have dared to do it if I did not feel myself sheltered by the authority of that great man who was guided by a never-erring instinct of truth. I thought that the best I could do for his memory was to recall to the minds of the men, by the energy and intelligence of whom chemistry has undergone its modern astonishing development, what important treasures of knowledge lie still hidden in the works of that wonderful genius. I am not sufficiently acquainted with chemistry to be confident that I have given the right interpretation, that interpretation which Faraday himself would have given perhaps, if he had known the law of chemical quantivalence, if he had had the experimental means of ascertaining how large the extent, how unexceptional the accuracy of his law really is; and if he had known the precise formulation of the law of energy applied to chemical work, and of the laws which determine the distribution of electric forces in space as well as in ponderable bodies transmitting electric current or forming condensers. I shall consider my work of to-day well rewarded if I have succeeded in kindling anew the interest of chemists for the electro-chemical part of their science.

MANUFACTURE OF SODA FROM SULPHATE.—Salt-cake is produced in quantity in California in the manufacture of nitric acid. As coal and lime-stone are dear in California, Le Blanc's process is not economical. The author therefore proposes to mix a solution of salt cake with calcium sulphite and pass in sulphurous acid. Soluble calcium bisulphite is formed, and by decomposition calcium sulphate and sodium bisulphite. The two salts are separated by filtration, and the sodium bisulphite is treated with milk of lime. The result is a solution of caustic soda, retaining a certain quantity of sodium sulphite and sulphate, which is evaporated down in the usual manner, and calcium sulphite, which is used again in the process.—J. PUTZKOW.

THE YELLOWSTONE NATIONAL PARK.

ANNUAL REPORT OF THE SUPERINTENDENT OF THE YELLOWSTONE NATIONAL PARK—to the Secretary of the Interior, for the year 1880.—Washington, 1881.

The chief feature of interest found in this report is a description of what is termed the Hoodoo Region, which is a terribly broken and eroded portion of the head branches of the East Fork of the Yellowstone and the Passamaria or Stinking Water Fork of the Big Horn, and which until this occasion had never been visited by tourists or government explorers; all previous information having been derived from a small party of prospecting miners, two of whom were killed by the Indians.

Mr. P. W. Norris, the Superintendent of the Park, having arrived on the ground with his party, some made sketches of the many weird wonders of erosion, copies of which may be found on another page of this issue, and Mr. Norris with an attendant took the elevations of the adjacent peaks, including Hoodoo Mountain. The latter was found to be 10,700 feet high (anneroid-barometer measurement).

This mountain is about a mile in length, and must present a most extraordinary appearance, and while probably itself not a crater, it is evidently of volcanic origin, and was eroded in its present form. Its southern face is still changing, here extending from 500 to 1500 feet below the summit; the frosts and storms of untold ages in an Alpine climate have worn about a dozen labyrinths of countless deep, narrow, tortuous channels amid the long, slender, tottering pillars, shafts and spires of conglomerate breccia and other remaining volcanic rocks. In shape they are described as being unlike any elsewhere known, being a cross between the usual spire and steeple form, and the slender-based, and flat, tottering, table-topped sandstone monuments, near the Garden of the Gods, in Colorado. And while lacking the symmetry and beauty of these, surpass both in wild weird fascination. Here the sharp-cornered fragments of rocks of nearly every size, form and formation, and shade of coloring, by a peculiar volcanic cement attached sideways, endwise, and upon the tops, sides, and apparently unsupported upon each other, represent every form, garb and posture of gigantic human beings, as well as of birds, beasts and reptiles. In fact nearly every form, animate or inanimate, real or chimerical ever actually seen or conjured by the imagination may here be observed. With this description and the illustrations we have given on page 187, some idea may be formed of the wild unearthly appearance of these eroded Hoodoos of the Goblin Land. These monuments are from 50 to 300 feet in height, with narrow tortuous passages between them, with sometimes tunnels, where the Big Horn sheep hide in safety; while the ceaseless but ever changing moans of the wild winds seem to chant fitting requiems to these gnome-like monuments of the legendary Indian Gods.

We have not sufficient space to allow us even to briefly follow Superintendent Norris in his very interesting description of the many wonders of this extraordinary region—the Yellowstone Lake, Geysers, cold, hot and medicinal springs, pulsating Geysers, terrace building springs, fossil forests, natural bridges, gold and silver mines, and many objects of scientific interest. Among the animals still to be found in the Yellowstone Park, mention is made of the bison, or mountain buffalo, which differs considerably from the bison of the plains; also the moose, elk, white tailed deer, black tailed deer, antelope, big horn sheep, bears, mountain lion or cougar, wolves, foxes, skunks, badgers, rock dog, porcupine, rabbits, rats, mice, moles, squirrels, chipmunks, beavers, otters, etc., etc.

We note the presence in trout, found in the cold water tributaries, of a "worm" named by Dr. Leidy "*Dibothrium cordiceps*." They are described "as long, slender, white worms, found in the intestines and flesh of the countless large and beautiful trout of the Yellowstone Lake, named by Professor Cope, *Salmo pleuriticus*." They are said to be entirely different to the worms found in European trout. The Superintendent does not appear to have succeeded in tracing the cause of this parasite, but states that they are only met with in fish found in the Yellowstone Lake. Here the trout exist in great numbers in water bubbling with hot gases; and the angler, without changing his position, or removing the fish from the hook, can rapidly boil them in seething pools.

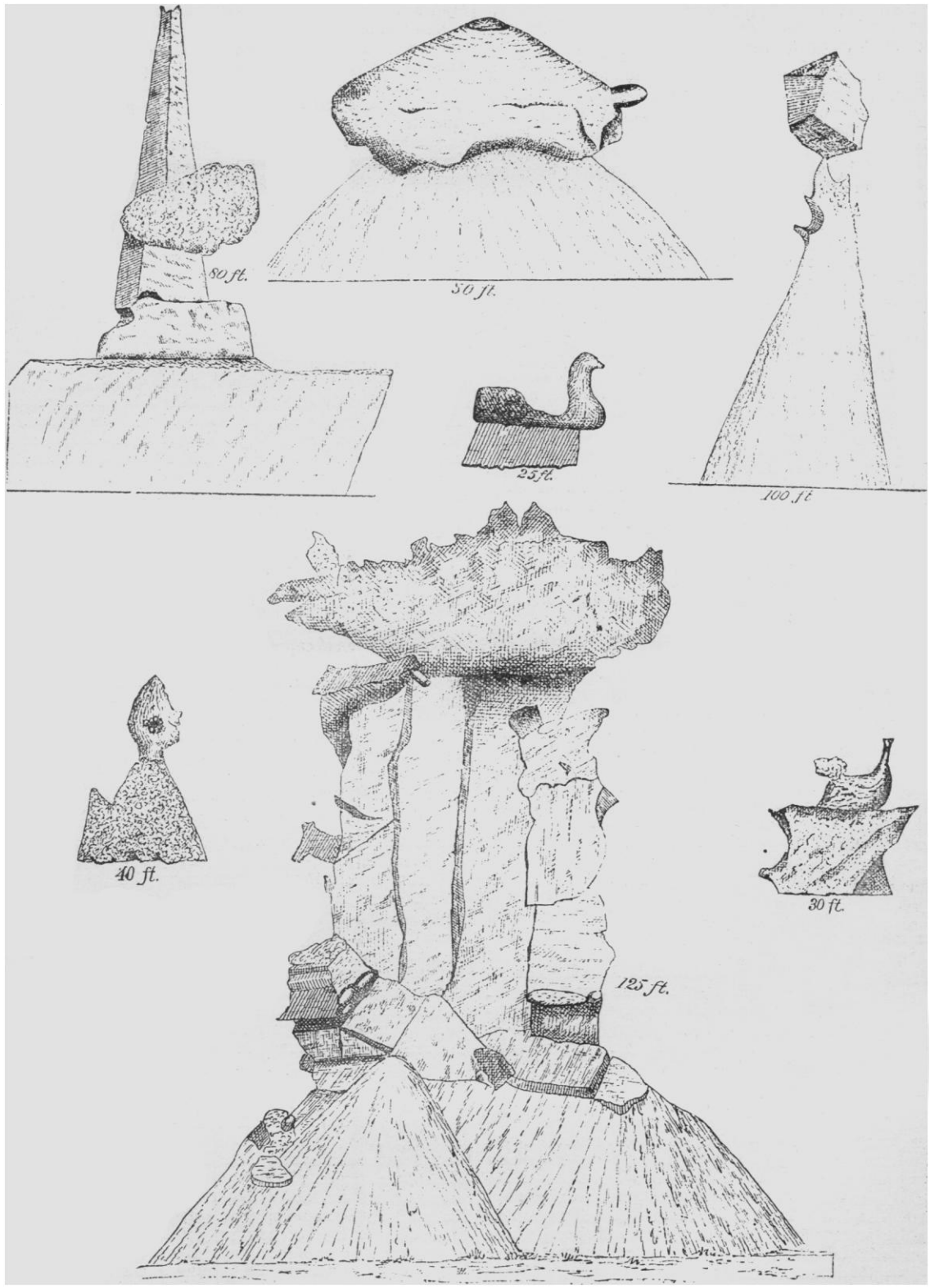
This National Park of the United States was visited by over 2000 tourists during the season previous to this report, all of whom returned in safety, although much inconvenience was experienced from the condition of the roads. An appropriation is now annually made for the improvement of the Park; and no one reading this report can fail to come to the conclusion that Mr. P. W. Norris is a gentleman highly qualified for the position of Superintendent, and brings an enthusiastic devotion to bear on his arduous duties in developing this "peerless wonder-land of earth."

Statement showing the *mean temperature* at BOSTON, MASS., for *each month* and *year* from *January*, 1871, to *December*, 1880, inclusive, as recorded at the *station of observation* of the *Signal Service, U. S. Army*, at that place.

[Compiled from the records on file at the office of the Chief Signal Officer, U. S. A., Washington, D. C.]

YEAR.	MEAN TEMPERATURE.												
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Year.
1871	27.2	29.4	42.8	46.7	57.4	66.2	71.0	71.8	59.7	54.2	39.8	38.4	49.5
1872	27.5	28.5	26.4	46.3	57.2	67.6	74.4	71.8	63.8	52.1	40.7	24.1	48.4
1873	26.3	27.6	34.1	44.6	56.9	67.2	72.9	68.8	61.7	53.0	33.4	32.9	48.3
1874	31.2	27.9	35.1	39.1	56.1	65.8	72.2	67.7	63.8	53.4	41.5	31.9	48.8
1875	20.9	22.8	31.7	42.2	57.3	66.5	71.7	69.7	58.8	49.3	34.4	29.8	46.3
1876	30.5	27.5	32.9	43.2	53.9	67.6	73.1	69.5	58.9	48.0	40.8	22.3	47.3
1877	24.2	33.6	35.1	44.3	54.9	66.5	69.9	70.7	63.9	51.3	43.8	36.1	49.5
1878	28.3	31.0	39.5	47.2	55.3	64.2	72.7	68.1	62.9	55.3	39.9	29.6	49.5
1879	24.5	24.5	33.8	42.4	59.4	64.2	69.9	67.7	60.8	56.6	39.2	32.6	48.0
1880	35.0	32.2	33.1	45.9	62.7	67.7	71.1	68.9	64.1	50.8	37.5	26.2	49.6

WAR DEPARTMENT,
OFFICE OF CHIEF SIGNAL OFFICER,
WASHINGTON, D. C., *April* 12, 1881.



HOODOOS OR REMNANTS OF EROSION IN THE GOBLIN LABYRINTHS.

YELLOW STONE NATIONAL PARK.

NOTE ON THE SENSORY TRACT OF THE BRAIN.

By Edward C. Spitzka, M. D., New York City.

It is well known that Meynert (1) and those who followed that distinguished anatomist, believed that the tract through which the conscious sensory impressions reach the cortex, extends from the columns of Goll and Burdach, of the cord and lower oblongata, through the so-called superior or sensory decussation, to the anterior pyramids; that thence the tract runs with the anterior pyramids in their outermost third through the pons and pes pedunculi, courses between the thalamus and lenticular nucleus in the posterior third of the internal capsule, and arching back, terminates in the cortex of the occipital lobe. Flechsig showed that what Meynert interpreted as the sensory pyramidal decussation, has no connection with the anterior pyramids, but, on the contrary, enters the lemniscus layer, or interolivary strand, whose relations to the corpora quadrigemina had been explained by Meynert, although he was befogged as to its lower relations, owing to the aforesaid confounding with the anterior pyramids proper.

Now, Flechsig (2) distinctly states in his work that the explanation he has been able to furnish of the real nature of the superior decussation, demonstrates the non-existence of a direct tract from that decussation to the cortex.

The true tract has, however, been known to exist, although the relations have not been properly interpreted. The lemniscus layer is not only a detachment from the corpora quadrigemina, but also distinctly incorporates a peculiar bundle, described by Henle as a fasciculus, from the pes to the tegmentum(3). This tract continues, in at least a part of the fibres, from the columns of Goll and Burdach to the pes pedunculi and thence, no doubt, to the cortex of the brain. The circuit for the conscious sensory impressions transmitted by the cord, and proposed by Meynert, therefore becomes re-established, with a modification, namely, that the sensory tract does not run through the pyramids and pons, but immediately above them, and after entering the *pes pedunculi* probably takes the course claimed by Meynert.

That there is a close relation between the pyramidal tracts and the by-track from the superior decussation to the *pes pedunculi*, is proven by an interesting observation which I have been able to make on the elephant's brain. In this animal(4) the entire pyramidal tract takes the course of the by-track, that is, there are no vertical fibres in the pons. The crus is continued bodily above the latter (which is composed exclusively of transverse fibres) to take the usual course on the ventral and medial aspect of the olivary nucleus.

This fact strengthens the proposition of Meynert, that there intervenes a third projection series between that of the tegmentum and that of the pes pedunculi, for which he proposes the name of the *stratum intermedium* (5). In man, I believe this stratum intermedium to be the main tract for the conveyance of conscious sensory impressions from the general sensory periphery, while in other animals, at least in the elephant, it is at the same time the voluntary motor tract.

That the sensory fibres occupy the most posterior portion of the internal capsule, while they compose the most dorsal in the pes pedunculi, shows that the fibres of the latter must pursue a spirally twisted course betore entering the brain. Such an arrangement seems to be indicated, indeed, in the outer contours of the crus. In an early human embryo, of about the third month, I find a well marked columnar elevation running from the outer part of the crus through the pons, where it touches its fellow of the opposite side, and then passes between the olives(6). This I regard as the embryonically distinct stratum intermedium.

(1). Das Gehirn der Saügethiere, in Stricker's Histology.
(2). Die Leitungsbahnen des Gehirnes und Rückenmarks. 1875.
(3). Lehrbuch der Anatomie des Menschen. 1872.
(4). "Science," February 7, 1881. (5). Archiv fuer Psychiatrie. 1874.
(6). Demonstrated before the N. Y. Neurological Society, March 1, 1881.

ASTRONOMIAL MEMORANDA.

A small pamphlet containing notes, corrections, etc., to the "Handbook of Double Stars," has been recently prepared by Messrs. Crossly, Gledhill and Wilson. In the introduction, the editors say: "The corrections have been thrown into two classes: the first contains those which from their importance demand immediate attention in order to save waste of time. These the reader is requested to insert at once. In the second list will be found a large number of corrections which may be entered as the stars are observed or read.

A very copious set of additional notes has also been drawn up, embodying, so far as we know them, the most recent and improved orbits, measures and discoveries.

It seems probable that the asteroid, No. 220, discovered by Palisa on the 23d of March, is identical with No. 139, Juewa. Juewa was discovered by the late Prof. Watson while engaged upon one of the transit of Venus parties in 1874 at Pekin. The asteroid was observed by Rümker at Hamburg, on November 8th of the same year, but since that date it has not been seen.

Nature for March 17, contains the following note upon the largest refractor in the world. "A very interesting scientific work, the most important of its kind yet attempted in the kingdom, has just been completed. It is the great refracting telescope, constructed by Mr. Grubb, of Rathmines, Dublin, for the Austro-Hungarian Government, and it is to be placed in the Observatory at Vienna. A commission appointed by the Government to examine the work, transmitted yesterday to the Austro-Hungarian Embassy, in London, a report expressing their full approval of the manner in which the task has been completed. It is a matter of no little pride to Ireland that she has produced the largest refracting as well as the largest reflecting telescope in the world." The object glass of this instrument is 27 inches in diameter or 1 inch larger than that of the Washington Refractor made by Clark.

W. C. W.

Washington, D. C., *April* 21, 1881.

INTRA-MERCURIAL PLANETS.

In "Science" of February 26, appeared an article on the above subject by "W. C. W.," which I have read with considerable personal interest, wherein we are led to infer, from purely negative testimony alone, that no such objects were seen during the total eclipse of Aug. 29, 1878, either by the late Prof. Watson or myself. Unfortunately, Prof. Watson's tongue and pen are now silent, and no one exists to defend his observations. What he has written on the subject the astronomical world is familiar with. It is about my own I wish to speak, and in defending them against the negative testimony which your correspondent brings, I hope to be able to convince the reader that because the observers whom he cites saw no planets, it is very far from proving their non-existence.

If the reader will refer to the article itself, he will find delineated on a chart the ground swept over by six observers, but he fails to tell us how short a time was devoted to a search west of the sun, and especially in the immediate region of the two objects seen by me, and near which one of Watson's objects was, viz., near θ Cancri. As not one in a thousand of your readers will have the privilege of reading the reports of those six observers, just published by the Naval Observatory, and, are therefore incapable of forming a correct conclusion on the subject, I have thought it advisable to quote what they really say, and, to remark, that when negative testimony is arrayed against positive, it is very important that its weight, if it has any, be carefully considered.

First, let the fact be stated, that during the total phase of the eclipse which lasted but 162 seconds, two experienced observers, with telescopes in every way well adapted for the work, state with positiveness that each saw two objects not down on any star chart, and, that they were not there when the sun had sufficiently withdrawn to allow the locality to be re-observed. On the other hand, three observers who searched west of the sun, one in a cloudy sky, and two of the others poorly equipped, and, devoting but a few seconds to the search, saw nothing, not even θ Cancri, a star of the fifth magnitude, near where one of Watson's and both of my objects were seen. The weakness of this negative testimony will be apparent from a few extracts from their reports.

Mr. Wheeler (telescope 5 inch, power 100) says, he observed the second and third contacts (beginning and end of totality), also the Corona on both sides of the sun, saw with the naked eye Venus, Mercury and Regulus, observed carefully the several prominences, etc., and then says, "An unsatisfactory attempt was made to sweep for Vulcan. The time given to it was limited, as I was expected to observe all the contacts, and time was consumed in recording the second, and again in bringing the telescope into position for observing the third contact." Now when it is considered that he undoubtedly occupied several seconds in looking at the grand sight with the naked eye, and, that the power used was altogether too high, and of course, the field very small, the time devoted to the search for Vulcan could have been but a few seconds. Is it therefore surprising that Mr. Wheeler saw nothing of the objects seen by me? Only those familiar with the use of telescopes know how perplexingly difficult it is to bring a well-known object in the field of a telescope, using a power of 100.

Mr. Bowman (telescope 3 inch, power 30) says he searched *north* and west of the sun (my objects, also Watson's, were southwest), and that some time was lost (during totality) in exchanging the diagonal tube for the straight one, swept to the westward 5° or 6° in the declination of the sun, and then returning, shifted the declination just far enough *north* to clear the Corona and swept to the westward again, then returned to the R. A. of the sun and shifted to the proper declination just in time to observe the third contact. When it is considered how much precious time was lost in observing and recording in his note-book the time of second contact, changing tubes, and probably observing the eclipse for several seconds with his naked eye, which he could hardly refrain from doing, is it at all wonderful that Mr. Bowman saw nothing of my objects or Watson's either?

Prof. Todd (telescope 4 inch, power 20) says, "I searched 15° each side of the sun, but the sky was *cloudy*, so much so that I was unable to see Delta Cancri," (a 4th mag. star). He does not say how much time he spent searching west of the sun. It certainly could have been but a moment, and, in the region where my objects were, but a few seconds. He, too, observed the second contact, also the Corona, saw Mercury, Venus, Mars, and Procyon. Again I ask is it at all surprising that Prof. Todd saw nothing of the objects seen by me?

Prof. Pritchett (telescope 3½ inch, power 90) says he first observed the grand scene with a naked eye, then swept along the ecliptic several degrees *each* side of the sun, observed all the phenomena of the eclipse, the second contact, Corona, the prominences, and the question arises how many seconds he searched with a very small field west of the sun for the "Ghost of Vulcan," as he facetiously calls it. Still again I ask is it at all wonderful that Prof. Pritchett saw nothing of the objects seen by me? Would it not, in fact, have been very surprising had he seen them at all?

Your correspondent has given in his diagram the outlines of the regions swept over by the above observers, saying: "The place of one of Watson's stars was covered by Wheeler, Bowman and Pritchett, and the place of Swift's two stars was examined by Bowman and Wheeler, and that one of the stars appears in the corner of Pritchett's sweep." Now all this is calculated to convey a wrong impression, for it is not likely that either of them knew within from 1° to 3° the exact boundaries of their hastily-made sweeps; neither do I pretend to be exact about the location of the stars I saw, although I made three estimates of their deviation and distance from the sun, by sighting along the outside of the telescope tube.

They are wrongly placed in the diagram. They were nearer where Theta is, and probably somewhat west of it, which would place it outside of the sweeps of all the observers. I should strongly suspect that one of them was θ, were it not that Watson, who says he saw that star, says nothing about another equally bright some 7′ from it, both ranging with the sun's centre.

Neither in his published statements, or letters to me, does he allude to this vital point. It was as impossible for him to have seen one and not the other, as for one to see Epsilon 4 Lyræ, without, at the same time, seeing Epsilon 5.

Again, he says, as far as relative position is concerned, my objects resemble closely d^2 Cancri, and B. A. C. 2810, on the *east* side of the sun. I hope he does not mean to be understood as inferring that it was on the east, instead of the west, of the sun I was searching.

Finally, he says, the existence of an intra-mercurial planet is not yet admitted by the majority of astronomers. This may be true, but I hope their opinion is based on stronger evidence than that adduced by "W. C. W."

LEWIS SWIFT.

ROCHESTER, N. Y., *April* 11, 1881.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of SCIENCE:—

The article on "Discrepancies in Recent Science" in a late number of this journal demands some attention, not because the Nebula Theory is seriously threatened by it, but because it properly calls attentions to some physical inferences that have been drawn from other phenomena and applied to the Nebula Theory, especially in the domain of heat. It is assumed by the writers quoted in that article, that *luminousness implies high temperature* and also that the rarity of the gaseous material of the nebula is the immediate result of the high temperature of the constituent atoms. Neither of these assumptions is correct. The trouble comes chiefly from the writer's failure to make the proper distinction between *energy* and *heat*, and I apprehend, also, in the failure to see clearly what the nature of heat is. Most of the books treat of this in a very loose way, and most of the statements on the subject by Mr. Charles Morris are wrong. How far wrong may be seen by comparing his statements with the following quotation from "The Mechanical Theory of Heat," by Clausius, Chap. 1st, Sec. X, p. 24: "*All heat existing in a body is appreciable by the touch and by the thermometer; the heat which disappears under the above changes of condition (fusion and vaporization) exist no longer as heat, but has been converted into work, and the heat which makes its appearance under the opposite changes (solidification and condensation) does not come from any concealed source, but is newly produced by work done on the body.*" We have all along been familiar with the conception of *heat as a mode of motion*, but not with the character of the motion except as "a brisk agitation of the molecules" or "a rapid vibration of the atoms;" but there are two kinds of vibratory motions possible to atoms, one of the character of pendulous motion or a

change of position in space of the centre of gravity of the atom, and the other the change of form of the atom itself; the first of these is known as free path motion, and the second as heat. The evidence for this may be briefly given.

First—It is certain that a heated body loses its heat by radiation, that is, it imparts its motion to the ether which transmits it in every direction as undulations having certain wave lengths and amplitudes. Second—It is certain that the energy of such undulations depends upon the amplitude of such undulations, and if the amplitude of the undulation was measured by the free path of the atom, then the radiant energy of the atom would vary as its free path, or in other words the rarer a gas is the greater its radiant energy. Now when the spectrum of a gas, say hydrogen, is examined, it is seen to be composed of lines having definite wave lengths, and wave length is dependent solely upon the rate of vibration. If this rate depended upon the number of impacts per second of the atoms or molecules of a gas, then these atoms would need to be always at exactly the same distance apart and the velocity of free path motion invariable, which conditions are physically impossible among free atoms, otherwise the spectrum we should obtain would be a continuous spectrum such as solid incandescent bodies give. But the spectrum of hydrogen for a given temperature is the same whether the gas be at ordinary pressure or very rare. This necessitates the conclusion that the heated atom which is thus radiating energy is vibrating quite independent of its position in space or of its free path motion, and the energy embodied in such vibratory motion is often spoken of as *internal energy*. When a swiftly moving bullet strikes a target, both bullet and target are heated and oftentimes a flash of light may be seen at the instant of impact. The free path motion has been changed into atomic vibrations, which at the first instant had a period capable of giving the sensation of light, but if the bullet be picked up at once it may not be uncomfortably hot. Now imagine two atoms in space urged by gravitation towards each other until they strike each other; each will be set vibrating, that is they will both be heated by impact, and until they were thus made to vibrate they would have no temperature at all; their energy would be represented by their free path motion; the greater their distance apart, when they began to approach, the greater would be their velocity at impact, and the period of vibration of each after impact would depend upon the character of the atoms themselves. One might have such a period as to give out undulations that might affect the eyes and we would say it was luminous while the other one might not, luminosity being dependent upon the rate of vibration, not upon the energy of vibration or the amplitude.

There are many phenomena, that are familiar enough, which show that luminosity does not depend upon high temperature. The decaying stump that shines at night, has a temperature not appreciably higher than surrounding objects; the swift moving molecules in a Crookes tube, that spend their energy upon the walls of the tube, cause the latter to glow, and the molecules themselves shine as they move in their long, free paths, but the tube is not uncomfortably hot, much less *very hot*. It is true that by increasing the energy of the moving atoms, the tube may be made red hot, but the point here is, that this is not essential for luminosity.

If then, in the process of universe building, we start with dissociated atoms, without any temperature,—at absolute zero, and let gravitation alone act among them, the first motions will be free path motions, and there will be no such thing as heat until atomic impact has begun; the energy that was at first represented solely by gravitation will now be partly changed into heat and radiation proper will begin, and the actual loss of energy to the involved atom will be greater than what would be due solely to gravitative approach; there might be luminousness with very little temperature, and one might speak of it as "fire mist," and as "glowing vapor," and yet not threaten the "Law of Interaction of Forces." Neither does the Nebula Theory fall, if originally matter was not hot, but cold.

TUFTS COLLEGE, MASS.

A. E. DOLBEAR.

DISCREPANCIES IN RECENT SCIENCE.

To the Editor of "SCIENCE:"

In his communication to your excellent journal (Vol. II., p. 142), Mr. Larkin has very correctly stated the discrepancy which is contained in the designation "fire-mist," as applied to the initiatory stage of nebular cosmogeny, the "Chaos" of Laplace—*sit venia verbo!* If the Nebular Hypothesis is a true representation of the history of our solar system (or all solar and other systems, for that matter) then, certainly, *heat* could have been present only after motion, and very lively motion at that, had been going on for quite a number of—well, let us say, billions of years, or pretty nearly that.

As soon as motion, *i. e.*, aggregation (and rotation) had begun, then, by the impact of the more distant portions of matter on those nearer the centre of the solar nucleus, heat was produced equivalent to the motion thus arrested. The primordial "Chaos," therefore, was cold and dark, if it ever did exist at all.

Mr. Larkin, consequently, is correct: There *is* a discrepancy!

Not so, Mr. Morris, whose objection is stated, *in nuce*, by himself (Vol. II., No. 41) in these words:

"Temperature and heat are very different things."

"It is one thing to contain heat and another thing to be in what we call a heated state."

To prove this he mentions the generally accepted facts "that a mass of water at 32° contains far more heat than an equal mass of ice at the same temperature; and a mass of water gas, (steam?) at 212° contains far more heat than an equal mass of water at that temperature."

The foregoing facts illustrate the phenomenon of "latent heat" or heat not appreciable by the thermometer. But *latent heat is not heat!* It is a misnomer that should have been eradicated from scientific nomenclature long ago. The heat which melts a pound of ice is employed in *performing* a certain amount of *work* by *overcoming* the *cohesion* of the solid ice. Its subsequent liquid state is the result of this work of heat. This heat has disappeared, is no more heat; exactly as the muscular force of the locksmith's arm disappears (is latent) at night, because by eight hours of filing he has overcome the cohesion of a quantity of iron. We can not look for the work and the force spent on it at the same time.

The greater mobility of the liquid and the diminished cohesion are the equivalent of the heat that has "become latent," *i. e.*, disappeared, absolutely, utterly and entirely, as heat. In changing water back again into ice, from the liquid into the solid state, the same amount of heat must be liberated, withdrawn, or allowed to escape, as was necessary to melt it.

Water, therefore, does not contain more *heat* than ice at 32° F.; it contains more mobility, energy, potentiality—in short, more *motion*, but not motion of the heat kind.

The same relations exist between water and steam at 212° F. Here the peculiar property of the gaseous condition allows us to appreciate the nature of the difference between water and steam much more precisely than that between water and ice. "Latent heat" is here simply *expansion*, and as expansion is the work of heat it is not heat. This we can prove by confining steam or any gas in a vessel with a movable wall. If the gas just fills the receptacle and we now apply heat, a thermometer will show a rise of temperature in the interior of the vessel.

As soon as the heat reaches a certain point, so that the

expansive force of the gas equals the resistance by the weight or friction of the movable wall, the latter will move and our thermometer, indicating the temperature in the interior of the vessel, will record *a fall of temperature*. Heat has "become latent," has disappeared, because it has done work, has moved the wall.

It is, therefore, not in accordance with the facts, if Mr. Morris states that a thing may be in a heated state and yet not contain heat. Nor is it true that "as density diminishes the heat capacity increases." The true state of things is evident from the examples given. Since diminution of density is effected by heat, is the work of heat, the gas thus expanded does not contain more, but less heat. The attenuation, the change of its state of cohesion, is the action of a certain amount of heat, and this heat has "become latent," has disappeared as heat, but it nevertheless exists in the expanded gas as a greater range of mobility, as diminished density. On condensation this heat again reappears, *i. e.* the range of mobility of the gas is diminished, and the motion, potentiality, energy or whatever name Mr. Morris should prefer to apply to this *causa efficiens*, is transformed into heat. This is, by the way, the very process that is supposed to have been going on in the Laplace "Chaos," and, therefore, proves again the discrepancy between the principle of the conservation of energy and the assumption that the primordial nebula was a "fire-mist," as Mr. Larkin has correctly explained.

As to Mr. Morris' conception of the action of gravitation, it seems still more erroneous. This gentleman says: "The earth must fall towards the body with the same energy that the body displays in falling towards the earth." Now, the two fundamental laws of gravitation, as first discovered by Newton are: Attraction acts in direct proportion to mass and in indirect proportion to the square of the distance.

The statement of Mr. Morris, therefore, is absolutely false.

Nor is this all. The possibility that gravity can act lies in the space given for the fall of a body acted upon. If, therefore, a body should fall, it must be raised first to allow it space in which to fall. If by some force 100 pounds are raised to the height of one foot, this body, if unsupported, will by its fall develop the very same force as was employed to raise it previously, viz.: 100 foot-pounds. In striking on some resistance, say the surface of the earth, it will develop an amount of heat, equivalent to its mechanical force 100 foot-pounds = 100 calorics or (small) heat units. Mr. Morris ignores the element of space, in so far as it must always have been previously furnished. He says:

"The motion that exists in a falling body was not created for the purpose. It existed in the falling body in some other form and has simply been transformed, not created."

But nobody ever contended that it was created. The possibility of its fall was given to the body by some elevating force, and it is the very same force which, having existed as potentiality to fall, as long as the body was supported, is transformed into motion, into falling, as soon as the support is withdrawn. And this fall is therefore not due to "internal forces" but to gravity and the space furnished by previous raising.

But is the discrepancy laid bare by Mr. Larkin the only one existing in regard to the Nebular Hypothesis? Nay, is this hypothesis really the true history of the world, and of our planetary system in particular? Is it as free from uncertainties and as little at variance with observed facts, as an hypothesis, which has found such ready credence and such universal approval, should be?

I believe it has no claim to such confidence as has been bestowed upon it, and since I have gone so far, I might just as well state a few reasons for my conviction, which I am glad to say is shared, or rather also held, by one of our first astronomers. This I shall do further on, but at present would beg to call the attention of your readers to two other discrepancies which exist between the principle of the conservation of energy and the Nebular Hypothesis—assuming, for argument's sake, the truth of the latter.

They may be best stated by two questions, to wit:

1. What existed before "Chaos," and how was "Chaos" brought about?

2. How long and by what influence did "Chaos" remain at rest, and what cause acted on it to force it into formative action?

I have never been able to get a satisfactory answer to these questions, nor have I been successful in answering them myself. They seem to convey the impression that a most glaring discrepancy with "conservation of energy" exists in this matter, of which every reader may become convinced on mature reflection.

For, in the light of the principle of the conservation of energy, motionless matter is an impossibility, as it is totally inconceivable to our understanding in every aspect. Again: Why should attraction—if we suppose that it was dormant while the primordial nebula was forming—suddenly begin to act? What force, what cause, instigated this action at a particular moment?

It has been said, and truly, I think, that it is a very cheap and easy matter to write upon the Nebular Hypothesis. But is it also an easy thing to write *against* this hypothesis? From the fact that it is rarely done, it would appear that it is not; and yet there are dozens of facts and arguments to be brought against it. Of these I will only name the more prominent ones, without going into detail:

The eccentricity of all planetary and lunar orbits.

The various deviations of the planes of all the planetary and lunar orbits from each other.

The retrograde motion of the moons of Uranus and of that of Neptune.

The composition of the rings of Saturn.

The greater velocity of revolution of the innermost portions of the rings than that of Saturn's surface.

The greater velocity of revolution of the inner moon of Mars than that of the latter's surface.

The immense number of comets and meteorites; their great eccentricities; the considerable number that have retrograde motion; the absence of any planetary nebula, so-called, which would allow of being interpreted as an initiatory stage of the formation of a solar system.

The multiple stars, etc.

The elaboration of the various facts above stated and several others* would far exceed the proper limits of this communication—which have, indeed, been rather overstepped already. I may be allowed, however, to add what Prof. Asaph Hall, to whom the paper described in the foot-note was submitted, wrote in reply:

"To me the Nebular Hypothesis is a very doubtful thing. The facts you mention are against it. Possibly its supporters may fudge it so that it will last a little longer, but it is always unsafe to rest on a theory based largely on our ignorance."

As may be imagined, I was very agreeably surprised by this card and asked Prof. Hall's permission to make use of it in case occasion should offer. This permission the excellent gentleman first declined to give; his reasons for so doing were stated in the following language:

"Such questions as the Nebular Hypothesis will only be decided by the slow growth of observation and knowledge and not by the opinion of this man or that. I have but little faith in it."

After some further expostulation on my part, this permission was kindly given in a letter, written December 9, 1879. Not, however, until now have I made any use of

* This I have done in a paper published in the November and December issues of the *Gæa* of the 1878, edited by Dr. Hermann Klein, at Cologne, Germany.

it, except to refer to it in a general way.* I am glad that the discussion arising from Mr. Larkin's letter has furnished such opportunity, and avail myself of it to give it to "SCIENCE" for publication. GEO. W. RACHEL.

NEW YORK, *April* 11, 1881.

MICROSCOPICAL NOTES.

Recent investigations respecting the pathological relations of diphtheria, and the discovery of a micrococcal organism in the false membrane, have made it almost certain that the morbid poison which gives rise to the disease is a parasitic organism. M. Talamon now states that he has succeeded in finding this organism in eight cases. In the condition of complete development they presented a characteristic mycelium and spores. The former are tubes with partitions from two to five thousandths of a millimetre in length. These under favorable circumstances, elongate and bifurcate, the bifurcations being characteristic in consequence of their incurved branches, like the sides of a lyre. In other conditions the mycelia do not become elongated, although they multiply so rapidly as to cover the surface of the cultivated liquid; they remain short and assume irregular forms, and give rise to numerous straight rods. The spores are of two kinds, round or oval, which may be termed the spores of germination, and rectangular spores or conidia. The latter characterize the species. They form small rectangles of various sizes, their length being sometimes fifteen thousandths of a millimetre. They may be isolated or united in festoons or zigzag chains. At first homogeneous they soon become filled with small round granules, highly refracting, and of the size of ordinary micrococci. The round or oval spores are those which by their elongation constitute the mycelium. They appear as clear points, from three to five thousandths of a millimetre in diameter, in the middle of a mass of granular material.

Animals and birds inoculated with these organisms all died developing the characteristic false membrane.

These facts are very important, and open up an entirely new field of investigation, and M. Talamon already hints that he has a clue as to the source from which the organism is derived in the case of human infection. We trust that those of our subscribers who possess microscopes will follow up the researches of M. Talamon, which promise results of the highest value to science and to humanity.

NOTES.

THE bicarbonate of soda prepared by the Solvay process contains from two to three per cent. ammonia, and is therefore not suitable for pharmaceutical use, and for certain technological purposes.

A NOVEL FERRIC HYDRATE.—C. Graebe has received from the Baden Aniline and Soda works a reddish crystalline substance deposited in cast-iron vessels in which potash has been melted. It has the same composition as hematite and gœthite, but its specific gravity is only 2.93.

CHLORINATED DERIVATIVES OF CARBAZOL.—On treating carbazol suspended in acetic acid with chlorine, the liquid turns blue, yellowish, greenish, and, lastly, red. If the reaction is then interrupted the product is trichlorocarbazol in white needles, melting at 185°. If the process is continued for ten or twelve hours, hexachloro-carbazol is produced, fusible at 225°.—W. KNECHT.

PRESENCE OF ALCOHOL IN THE SOIL, THE WATER AND THE ATMOSPHERE.—A. Muntz has previously shown that it is possible to detect exceedingly slight traces of alcohol by converting it into iodoform. On concentrating the alcohol in a small volume of water by means of fractionated distillation, and using the microscope to verify the presence of iodoform, he was able to recognize with ease 1-300,000th of alcohol mixed with water. He has since improved the process so as to detect quantities even smaller than 1-1,000,000. During the last four years he has applied this method to river, spring, and sea-water, as well as to rain and snow. The results obtained leave no doubt of the presence of a neutral body, more volatile than water, and yielding iodoform. He thinks alcohol is the hydro-carburetted body present in the atmosphere, indicated by the researches of Boussingault and De Saussure. Soils rich in organic matter yield alcohol in such proportions that its essential properties may easily be verified.

NEW SYNTHESIS OF DESOXY-BENZOIN AND CRYSENE.—MM. Graebe and Bungener have obtained desoxy-benzoin by causing the chloride of phenyl-acetic acid to react upon benzol in presence of aluminium chloride. By the same reaction, naphthalin being substituted for benzol, they produce benzyl-naphthyl-keton. They then reduce with hydriodic acid and phosphorus at 150° to 160°, and pass the vapors of the carbide thus obtained through a red-hot tube, when 4 atoms H are removed and chrysene remains.

CERTAIN PHENOMENA OF OPTICS AND OF VISION.—M. Trève mentions the fact that the flame of a lamp appears brighter, and that a vertical shaft, a post, or mast is seen more distinctly through a vertical than through a horizontal slit, whilst a house, a landscape, or the disk of the sun or moon is perceived more clearly through a horizontal slit. He finds similar differences in photographs according as the light passes from the object to the plate through a vertical or a horizontal slit, and ascribes the results to the action of diffused light.

COPAL varnish for mounting objects for the microscope has been suggested by Mr. Julien Derby of the Quekett Club, who states that Mr. Van Heurck, of Antwerp, who first used it, has met with much success in mounting diatoms with that medium. This varnish is used about the consistency of oil and should be of that brand known as "pale copal." It has about the same refractive index as balsam, and is free from bubbles. Drop the copal over the object and slightly heat over a spirit lamp. In some cases a cover can be dispensed with, as it soon takes the consistency of amber, and is hard enough to sustain wiping and brushing with a soft brush with impunity.

WIDENING OF THE RAYS OF HYDROGEN.—The nebulous expansion of the spectral rays of hydrogen, noticed on increasing the pressure of this gas in a Geissler tube, is still ascribed to the influence of the pressure, though Dr. Shuster, Secchi, and others have shown that it is not possible to alter the pressure of a gas without at the same time affecting the resistance of the medium, and in consequence the temperature of the spark which traverses it. C. Fievez has undertaken to examine separately the influence of the different agents, temperature, pressure, direction of the current, etc., which have been suggested as contributing to produce this phenomenon. He finds that the widening of the hydrogen rays is correlative to the rise of temperature. We may affirm that the temperature of a celestial body is higher than that of another if its hydrogen rays are broader.

RECIPROCAL DISPLACEMENTS OF THE HYDRACIDS.—The action of the hydracids upon the salts formed by the halogens is in general the inverse of that of the elements themselves. Thus hydriodic acid expels hydrochloric acid from the metalic chlorides and hydrobromic acid from the bromides, whilst hydrobromic acid also liberates hydrochloric acid from the chlorides. The chlorides in general are decomposed by hydrobromic acid, and this decomposition preponderates according to the thermic value of the principal action. But the bromides may also be decomposed, though less readily, by hydrochloric acid. This inverse action previously pointed out by M. Hautefeuille in the salts of silver at a red heat, and by the author in the moist way, has lately been observed anew by M. Potilizine, but it is in no way contrary to thermo-chemical principles. It results from the existence of secondary compounds, partially dissociated, which intervene with their peculiar heat of formation. The theory of these reciprocal actions and equilibria is always the same. In every case we have to do with a principal re-action, foreseen by the thermic theory, and a perturbation equally foreseen by the same theory, of which it is a necessary confirmation.—M. BERTHELOT.

*"SCIENCE," Vol. I, p. 246, foot-note to the paper on Friedrich Mohr's Life and Works; *Scientific American Supplement* No. 266, p. 4,241, in a paper on "The Actual Figure of the Earth."

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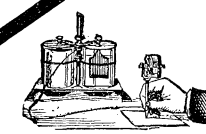
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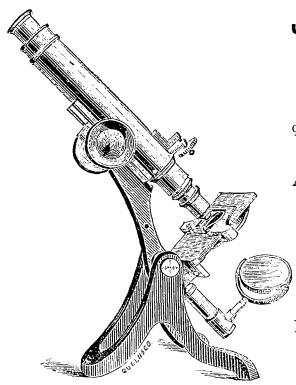
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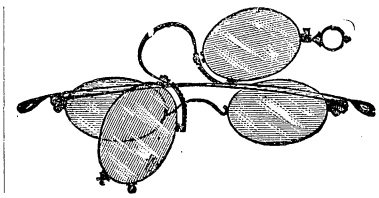
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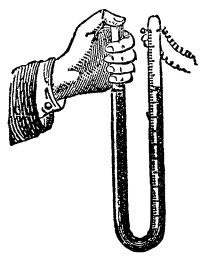
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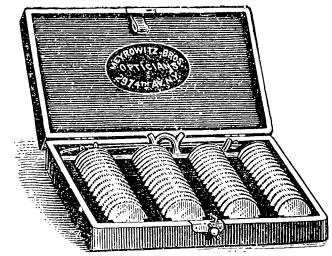
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